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SCIENCE

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CONTENTS

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A SURVEY OF AMERICAN BIOLOGICAL CHEMICAL LITERATURE[1]

ABOUT a year ago Sparks and Noyes[2] prepared a census of the periodical literature of chemistry published in the United States. They selected for their study the five American chemical journals:

American Chemical Journal
Journal of the American Chemical Society
Journal of Biological Chemistry
Journal of Industrial and Engineering Chemistry
Journal of Physical Chemistry

This study showed that during the five-year period, 1909–1910 to 1914–1915, the *Journal of Biological Chemistry* gained 150 per cent. in the number of published pages; the *Journal of Industrial and Engineering Chemistry,* 78 per cent.; the *Journal of the American Chemical Society,* 88 per cent.; while the *Journal of Physical Chemistry* showed a loss in published pages of only 2 per cent. (*The American Chemical Journal* was merged with the *Journal of the American Chemical Society* in 1914).

With this study as a model and an incentive, the writer has made a somewhat similar investigation of the biological-chemical literature during the period 1907–1916. The question as to what properly belongs under the heading biological chemistry is probably open to discussion. It seems fair, however, to accept the decision of the men who are in charge of the various sections of the Biological Chemical division of *Chemical Abstracts.* All papers which are found in

[1] This study was presented before the Second Annual Conference of Biological Chemists, held at the Chemist Club, New York City, December 31, 1917.

[2] Sparks, Marion E., and Noyes, W. A., SCIENCE, 1917, 45, 168 (February 16).

this division have been considered in this study.[3]

The first indication of the increased interest in biological research may be found in the large number of publications which have been founded and successfully continued during this period. The principal periodicals published prior to 1907 are given in Table I. The date indicates the year of the first publication.

TABLE I

Publications of Biological Chemical Interest Prior to 1907

American Journal of Medical Sciences	1827
Journal of the American Medical Association	1883
SCIENCE	1883
Journal of Experimental Medicine	1896
Journal of Medical Research	1896
American Journal of Physiology	1898
Biological Bulletin	1899
American Journal of Anatomy	1901
Journal of Infectious Diseases	1904
Journal of Experimental Zoology	1904
Journal of Biological Chemistry	1905
Anatomical Record	1906

Those journals founded during the period 1907–1916 are given in Table II.

TABLE II

Periodicals of Biological Interest Founded During the Period 1907–1916

Chemical Abstracts	1907
Archives of Internal Medicine	1908
Journal of Pharmacology and Experimental Therapeutics	1909
Biochemical Bulletin	1911
American Journal of Diseases of Children	1911
Journal of Agricultural Research	1913
American Journal of Tropical Diseases	1913
Journal of Laboratory and Clinical Medicine.	1915
Journal of Parasitology	1915
Journal of the Association of Official Agricultural Chemists	1915
Journal of Cancer Research	1916
Journal of Bacteriology	1916
Journal of Immunology	1916
Quarterly Cumulative Index to Current Medical Literature	1916
American Journal of Syphilis	1917
Abstracts of Bacteriology	1917
American Review of Tuberculosis	1917
Journal of Urology	1917

[3] It should be remarked in this connection that chemistry is only one of the sciences which have been used in the study of biological phenomena. A similar study might be made for each of the other sciences, especially physics.

The number of journals which have sprung into existence since 1914 is very noticeable. Many of these probably owe their existence, in part at least, to the fact that publication in German magazines was cut off by the war. It is to be hoped that they may survive after the war, when many scientific investigators will again be tempted to publish in German.

Special attention should be directed to the two abstract journals. *Chemical Abstracts* is by far the most complete abstract journal published in any language. It covers nearly 700 periodicals (671 titles are given in the 1915 list; many have been added since then, as the 1917 list, shortly to appear, will show. Still others are covered through other abstract journals, about a dozen of which are regularly checked). The *Biological Abstracts,* so well organized by Professor Gies, and carefully edited by his staff, are especially complete. Very few, if any articles of biological interest are missing. The fact that about 10,000 copies are in circulation must add to the value of American scientific research. *Abstracts of Bacteriology* covers a similar field for the bacteriologist and appears to be equally well organized. And finally, the *Cumulative Index of Current Medical Literature,* issued quarterly, gives in one index the current medical and biological literature. We are grateful for these publications, whether we express it or not. They lighten very much the burden of reference hunting.

The following journals have been used in the study recorded here:

American Journal of the Diseases of Children
American Journal of Medical Sciences
American Journal of Physiology
Archives of Internal Medicine
Journal of Agricultural Research
Journal of the American Chemical Society
Journal of Bacteriology

Journal of Biological Chemistry
Journal of Cancer Research
Journal of Experimental Medicine
Journal of Immunology
Journal of Infectious Diseases
Journal of Medical Research
Journal of Pharmacology

TABLE III

Relation of the Biological Articles to the Total Number of Articles Published, 1907–1916

Periodical	Pages	Articles		
		Total	Biological	Per Cent.
Am. J. Dis. Children	5,844	399	119	30
Am. J. Med. Sci.	—	1,351	168	12
Am. J. Phys.	12,135	864	500	58
Arch. Int. Med.	—	1,112	349	31
J. Agr. Res.	4,851	329	75	23
J. Am. Chem. Soc.	—	2,164	383	17
J. (Org. and Biol.)	—	994	383	40
J. Bact.	477	37	15	40
J. Cancer Res.	460	24	5	20
J. Biol. Chem.	14,384	1,304	1,304	100
J. Exp. Med.	11,648	885	398	45
J. Immunology	556	34	21	61
J. Infect. Dis.	—	692	289	42
J. Med. Res.	—	589	176	30
J. Pharm.	5,040	304	254	80

These were selected as most likely to contain the more important literature of biological-chemical interest. They do not contain all, by any means, as it is met with in the most unexpected places. It would be of interest to compile a complete list of all the work of biological chemistry published by the various American laboratories in all the periodicals, American and foreign. This the writer must leave to some one else.

Table III. contains a list of the total number of articles and the number of those of biological-chemical nature, and in some cases, the number of pages. As Sparks and Noyes pointed out, and as many others have remarked in discussing the subject, the number of pages has no real significance in many instances. Many short articles are of more value, scientifically, than other articles with their hundreds of pages. The length of an article depends too much upon the nature of the subject and the personality of the writer, and not enough upon its scientific value.

Table IV. contains a comparison of the number of articles (and pages in some instances) in those periodicals published during the periods 1907–1908 and 1915–1916. In those cases where the periodical was not published in 1907, the figures for the later period are given for comparison.

These tables bring out, rather forcibly, first, the large amount of biological work which is being carried out in this country, and second, the marked increase in this kind of research during the past decade. The curve will drop

TABLE IV

Showing Per Cent. Increase During the Ten-Year Period

Periodical	Pages			Articles Total			Articles Biological		
	1907–1908	1915–1916	% Increase	1907–1908	1915–1916	% Increase	1907–1908	1915–1916	% Increase
Am. J. Dis. Children	—	1,610	—	—	113	—	—	39	—
Am. J. Med. Sci.	—	—	—	257	275	7	18	49	17
Am. J. Physiol.	2,522	3,198	23	199	237	20	134	137	2
Arch. Int. Med.	2,468*	4,071	65	149*	252	69	38*	142	274
J. Agr. Res.	—	3,596	—	—	218	—	—	61	—
J. Am. Chem. Soc.	—	—	—	402	584	45	86	88	0.2
J. Bact.	—	477	—	—	37	—	—	15	—
J. Biol. Chem.	1,556	5,384	246	121	435	259	121	435	259
J. Cancer Res.	—	460	—	—	24	—	—	5	—
J. Exp. Med.	1,421	3,052	114	95	223	134	55	93	70
J. Immunology	—	556	—	—	34	—	—	21	—
J. Infect. Dis.	1,290	2,560	99	91	213	134	44	86	95
J. Med. Res.	2,881	2,062	—	125	128	2.5	35	55	57
J. Pharm.	—	1,715	—	—	117	—	—	103	—

* For the period 1908–1909.

somewhat during the period of the duration of the war, because of the large numbers of biological chemists and physicians who have already entered, and who will enter the government service. However, we may look for a return to the highest point of the curve as soon as normal conditions exist again. It would seem that one of the lessons we must learn from this terrible conflict is the national value of scientific research.

TABLE V

Institutions which Have Published Ten or More Papers in Any One Periodical

	Am. J. Physiol.	J. Biol. Chem.	J. Exp. Med.	J. Pharm.
Armour and Co.		10		
California	13	89	12	
Carnegie Nutrition	17	10		1
Exptl. Evolution	7	5		
Chicago	70	40	13	11
Columbia	21	23	30	6
Conn. Agr. Expt. Station	30	26		
Cornell Medical	25	46	3	7
Harvard	38	95	10	9
Health Dept., N. Y. City		3	10	
Herter Lab.		77		
Illinois	12	20	10	
Johns Hopkins	34	39	37	51
Mass. Gen. Hospital	1	16	1	7
Michigan	2	3	2	10
Missouri	4	14	2	3
N. Y. Agr. Expt. Station		35		
N. Y. Post Graduate		14		
Montefiore Home	2	13	1	
Northwestern	10	3		13
Pennsylvania	8	53	20	10
Rockefeller	16	135	114	8
Roosevelt		13		
U. S. Fisheries	5	19		
U. S. Hygienic Lab.		7		10
Vanderbilt	2		12	
Washington Univ.	6	18		6
Western Reserve	24	15	15	37
Wisconsin	14	57	7	9
Yale	35	117	11	8

Table V. contains a list of those institutions which have published ten or more papers (biological) in any one of the four periodicals tabulated, with the number of papers published in the others. (Lack of time prevented a complete classification of all the periodicals). Table VI. classifies the articles in these four periodicals according to the scheme used in *Chemical Abstracts*.

These tables and figures, incomplete as they are, give us an idea of the large amount of biological material which is being published yearly. As Professor Vaughan remarks about medical literature, some is good, some is bad, and much of it is indifferent.[4] While the direct responsibility for the quality of the work published depends upon the investigator himself, some of this responsibility must be laid upon the teachers of biochemistry of this country. This responsibility may even be carried back still farther, as Dr. Hammett[5] has recently pointed out, and may in part be placed on the shoulders of the teachers of the fundamentals of chemistry. It is a hopeful sign, in view of this responsibility, to see so large a number of the leaders of biological chemistry gathered in this conference for the improvement of its teaching. May the inspiration which is gathered here send us back to our desks and our laboratories with the determination to do our best the coming year to build deep and strong the foundations upon which the future biological publications will be based.

TABLE VI

Classification of Articles

	Am. J. Physiol.	J. Biol. Chem.	J. Exp. Med.	J. Pharm.
Organic	34	213	2	4
General Biology	58	196	2	1
Methods	16	180	8	14
Botany	0	17	1	4
Bacteriology	257	121	61	2
Physiology	29	171	69	62
Metabolism	63	265	24	2
Pharmacology	40	6	33	144
Pathology	21	38	166	17
Zoology	3	10	2	3

CLARENCE J. WEST

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

THE AGE AND AREA HYPOTHESIS

PROFESSORS SINNOTT AND BERRY[1] express themselves unfavorably to my hypothesis of "age and area," which Professor de Vries

[4] Editorial, *J. Lab. and Clin. Med.*, 1915–16, 1, 59.

[5] Hammett, F. S., SCIENCE, 1917, 46, 504 (Nov. 23); *Medical Record*, 1916, 90, 503 (Sept. 16).

[1] SCIENCE, N. S., Vol. 46, p. 457, November 9, 1917; p. 539, November 30, 1917.

has done me the honor to endorse in two reviews[2] in the same journal. But I may perhaps be permitted to point out that, though they object to the hypothesis, they put nothing in its place, and make no effort to explain the figures upon which it is based, and which are so clear and so consistent that if age and area be abandoned, or a rival hypothesis be set up, they can not be left without an explanation, The hypothesis was originally based upon the results obtained from the flora of Ceylon, where distribution was only estimated, and though there were many irregularities, the figures came out clearly enough for it to be fairly safe to publish the hypothesis. Since then, actual measurements of area occupied have been used—for New Zealand, the islands round New Zealand, Jamaica, Hawaii and Australia—and have given results which are far more clear and decisive, following in the most extraordinary manner what is required under the hypothesis. Its applicability has also been extended from the angiosperms to the Coniferæ and ferns.

Professors Sinnott and Berry, it seems to me, have to some extent misapprehended my views. They certainly give them too wide an application, and do not take sufficient notice of the provisos with which I have hedged round the use of age and area. For example, as I have several times pointed out, age and area must not be applied to single species (as Professor Sinnott applies it), but only to groups of 20 allied forms. In other words, it is primarily a proposition in taxonomic plant geography; and though I think that its application may probably be extended, I have as yet only applied it to groups of allied species within a single country, and to genera (the closest of groups) beyond.

I believe that *in general*, when there is a genus covering the range of its family, that genus is the parent of the rest of the family. But of course not infrequently there are two or more genera covering the family range, or two or more dividing it between them (*e. g.*, one Old World, one New). In the solution of this problem, with which goes the parallel fact that, as Professor Sinnott points out, genera are often represented in a given country only by endemic species, and the similar fact that species may be only represented by endemic varieties, there lies, I believe, a very large step on the way to modern theory of evolution.

There is no doubt that many species have in past times died out, or been killed out, but that is no proof that the process is going on now, though *man* is no doubt responsible for a great deal. Age and area, however, refers to action under practically unchanged conditions, and the advent of man may completely alter them.

Professor Sinnott so far extends the applicability of age and area as to make it responsible for the deduction that herbs are older than trees. In the first place these are *ecological*, not taxonomic groups, and the fact that they are undoubtedly very polyphyletic complicates the matter; and in the second place I have not said that the rate at which a herb and a tree spread is the same, though both are governed by age and area, and one covering 1,000 square miles bears the same age relationship to one that covers 500, in each case.

The attacks by Professors Sinnott and Berry are upon my first three papers on age and area.[3] Since then I have published[4] three more. The first is controversial, dealing with the inapplicability of natural selection, and with the question of relative age (in one country) of endemic and widely distributed species. I have shown by two crucial cases that the former are the younger. In New Zealand the widely distributed species take no notice of Cook's Strait (between the chief islands), while many endemics are held up there, having evidently arrived too late to get across before the formation of the strait. Again, in the Tristichaceæ and Podostemaceæ, the primitive genera, which resemble ordinary water plants, are spread throughout the tropics, while the extraordinarily dorsiventral forms, which

[2] SCIENCE, N. S., Vol. 43, 1916, p. 785; 45, 1917, p. 641.

[3] *Phil. Trans.*, 1915; *Ann. Bot.*, 1916.

[4] *Ann. Bot.*, 1917.

must be younger, are all local, and are no more numerous. I have shown in another paper[5] that one can not speak of adaptation in these plants, other than the first adaptation which enabled the families to live in running water.

Neither of my critics makes any attempt to explain why under my hypothesis one can make numerous predictions about the composition and distribution of a given flora—predictions which as yet have always proved to be correct on verification of the facts. For example, in the paper on New Zealand, to which they object, I predicted that the number of endemics found in each zone of 100 miles from north to south in New Zealand would show a maximum at some point (sometimes two) and a regular tapering away from this. This proved to be the case for every one of the 91 families and 392 genera—a result hitherto quite unsuspected, and which alone was almost enough to establish age and area in a higher rank than that of a mere hypothesis. In the second of my recent papers mentioned above I predicted that the species which reach the islands outlying round New Zealand (Kermadecs, Chathams, Aucklands) would on the whole be the oldest in New Zealand, and therefore very widespread there. This proved to be the case, not only generally, but in detail, the most widespread being those reaching all three groups of islands, the next those reaching two, then those reaching one, and finally those that were confined to New Zealand, however far they might range in the world in general. In this paper I have likewise shown that the area covered in New Zealand by the species that also reach Australia, etc., goes, not with the area covered in the world in general, but with that covered in the archipelago of which New Zealand is the principal part. This result seems to me to exclude any explanation based upon natural selection.

In the same paper it was predicted that the species endemic to New Zealand and the islands round it would also be very widespread in New Zealand. This proved to be the case; they are even more widespread than the average of the species which range beyond New Zealand to Australia and the rest of the world. This also is a fact that is quite inexplicable by natural selection.

It seems to me that a hypothesis that shows itself thus capable of being used as a basis for successful prediction deserves at least a very careful investigation before being rejected. One of the best proofs of its general applicability is the fact that other workers are beginning to apply it to the solution of various problems. Breakwell has already worked out the distribution of the grasses of Australia, and shown that it agrees with age and area. Small, in work now being published on the Compositæ,[6] has found it to confirm the evidence of other lines of work in phylogeny, and a third worker is applying it to the Leguminosæ.

In the last of the three papers mentioned above I have shown that the orchids of Jamaica are least widespread in that island when endemic to it, more widespread when also found in Cuba, and most widespread of all when ranging yet further than Cuba. It is also shown that the flora of the Hawaiian Islands fits the age and area hypothesis, as does the distribution of *Callitris* (Coniferæ) in Australia. The ferns of New Zealand and Hawaii are then considered, and shown to obey the same law, while confirming previous work on the greater youth of endemic species.

Finally, in two further papers, as yet unpublished, I have applied age and area in more detail. In my first (a reply to the criticisms of Professor Sinnott, who objects that age and area will not explain the flora of New Zealand) I have shown that New Zealand was peopled with plants in all probability by two chief invasions, one northern from Indomalaya, one southern. In the second paper I have dealt with the flora of Stewart Island (the southernmost of New Zealand) and have successfully made no fewer than 15 predictions about its composition and geographical relations, bringing out a number of points hitherto unsuspected or unnoticed.

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[5] *Proc. Roy. Soc.*, 1914.

[6] See his review in *Science Progress*, January, 1918.

THE RELATIVE AGE OF ENDEMIC SPECIES

IN my previous reviews of the studies of J. C. Willis on endemic species I have explained his thesis that they are, as a rule, the youngest components of a flora and that their special condition is due to the circumstance that they have not yet had time to spread. This conclusion was based chiefly on statistical studies of the angiospermous floras of Ceylon and New Zealand.

It was to be expected that this contention would not escape contradiction, since among the older botanists the opinion prevails that endemic species are everywhere the relics of an old flora which is now rapidly disappearing. There is no doubt that in some countries this may be the case, and that even in the islands studied by Willis some few species are in this condition. But the number of these relics is so small that whenever a flora is studied statistically, they have no visible effect on the figures, provided that the species are dealt with in groups of about twenty or more.

The arguments against the hypothesis of the relative youth of endemics have been collected and brought forward in a recent article by H. N. Ridley and replied to by Willis.[1] In the first place it is claimed that it is difficult to show from which other species of the same flora the endemics should have been evolved. But Willis answers that most of them have "wides" of the same genus in their neighborhood, the "wides" being those species which also occur in adjoining countries, and are usually widely spread. A study of the diagnostical differences of the endemics with these goes to prove that in most cases they can easily be derived from them. Moreover it shows that these characters are by no means of an adaptive nature since they do not betray any relation to the life conditions of the local environment.

Ridley is an adherent of the Darwinian principle that organisms produce varieties, which if more suitable to the surrounding condition than the parent form are selected. This thesis, of course, is the basis of all evolutionary theories. But the question whether the production goes by infinitesimal steps or by larger changes, makes the difference between the theory of natural selection and that of mutation. On this point his criticisms clearly show that the more narrowly one looks into the actual facts the larger becomes the evidence against the older view. The reader will find a valuable review of the arguments in the papers of both antagonists, but it would take me too far to consider them here.

The main point of Willis's position, however, has been left unattacked. It is the statistical result that the endemics and the widely distributed species in a country are arranged in graduated series, showing an increase in number in opposite directions, the endemics increasing from those of wide to those of narrower distribution, the wides in the other direction. Or, in other words, the endemics of a flora are the more numerous the smaller their area is, whereas among the species occurring also outside the special flora studied, those with a wide distribution within it, prevail. The regularity with which these facts appear from the tables made for different islands and different botanical groups can not be explained on the old view. Neither is this possible for the fact that endemics have mostly contiguous areas, whereas the dying out of species should lead us to expect the occurrence of their last relics on sundry spots and on distant points of their original habitat.

This law of distribution, which Willis calls his law of "age and area" is then tested for some other floras, besides those already mentioned.[2] The orchids of Jamaica give a very convincing instance. Dividing the island into 19 equal squares and comparing the endemic orchids with those found also in Cuba, and

[1] H. N. Ridley, "Endemism and the Mutation Theory," *Ann. Bot.*, Vol. XXX., 1916, p. 551, and J. C. Willis, "The Relative Age of Endemic Species and Other Controversial Points," in the same journal, Vol. XXXI., 1917, p. 189. See also some other articles of the same authors in that journal, and my reviews in SCIENCE, N. S., Vol. 43, No. 1118, pp. 785–787, June, 1916, and Vol. 45, No. 1173, pp. 641–642, June, 1917.

[2] J. G. Willis, "Further Evidence for Age and Area, its Applicability to the Ferns, etc.," *Ann. Bot.*, Vol. XXXI., 1917, p. 335.

with those of still wider distribution, the tables show that their range goes in the order: Endemics least, Jamaica-Cuba species next, wides greatest. Then the angiospermous flora of the Hawaii archipelago, which comprises seven islands, is tested, and it is found that the wides range much more than the endemics, of which more than one half is found on one island only. These latter must be the youngest, the remaining having originated before the splitting up of the region into separate isles.

A study of *Callitris* in Australia shows that the law holds good for Conifers also, and the fern floras of Hawaii and New Zealand give ample material to prove that the endemic species, although following the same rule, show a much greater range than the endemic angiosperms, a result to be expected on Willis's hypothesis, but contrary to what one would expect if endemics were dying out, since ferns are generally considered as a much older group than the flowering plants. A last argument is given by the outlying islands around New Zealand. Starting from his hypothesis Willis predicted that the most widespread plants in the two main islands would be those that reach the outlying isles also, and that those which do not reach them, are less widespread. The figures given in the tables bear out this fact in a striking way, both in the case of wides and endemics.

From these results it seems clear that all over the earth and in every systematical group of plants the rule prevails that the most widespread species are the oldest, whereas the others are the younger, the smaller their area is. This law would provide us with a new method of constructing pedigrees and of judging the relative age of diagnostic characters, and it seems evident that these points would be of paramount importance in the study of the real relationships and the common origin of species.

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SCIENTIFIC EVENTS

THE CHICAGO MEETING OF THE AMERICAN MEDICAL ASSOCIATION[1]

The sixty-ninth annual session of the American Medical Association, held in Chicago last week, was one of the most important the Association has ever held. This statement is made as the meetings, the exhibits, the addresses and the results of the past week pass in retrospect before us. The House of Delegates considered many topics of current war interest and passed a number of resolutions of important, timely character, conspicuous among them being those on animal experimentation, on universal military training, on welfare work among children, on the use of enemy manufactured pharmaceutic products, and on the work of Surgeon-General Gorgas.

The opening meeting of the Scientific Assembly, held in the Auditorium Theater, was greeted by an audience of over 4,500 persons, every seat and available space in the theater being occupied. Unfortunately, many who desired to attend were unable to find accommodations because of late arrival. The music for this session was provided by the Fort Riley Band, which was a conspicuous feature of the annual session, and aided in arousing military enthusiasm. At this meeting, as in all of the night meetings, the medical officers in uniform were seated on the stage, and added military tone and color to the picture. The scientific programs began on Wednesday and contained numerous papers of military interest, as well as those of a strictly scientific character.

An unusual feature of the session was the replacing of the president's reception by a medical war meeting held in Medinah Temple, the report of which appears elsewhere in this issue. The local committee on arrangements had done notable work in staging this meeting. Every seat in the immense auditorium was filled, over 6,000 persons being present. The speakers were the noted foreign guests, the surgeon-generals, the president of Leland Stanford University and Major Alexander Lambert, of the American National Red Cross. The enthusiasm of the audience can not be depicted by words, every speaker and patriotic enunciation being greeted with an ovation. With the introduction of each foreign guest the audience, led by a local choral organization and accompanied by the music

[1] From the *Journal* of the American Medical Association.

of an immense pipe organ and the Fort Riley Band, sang a national song.

On Thursday, practically all of the scientific sections combined in two meetings of the greatest importance to the Army, the profession and the public. These meetings were held in the Auditorium and Studebaker theaters, and concerned the reconstruction and rehabilitation of disabled soldiers and the physical examinations made under the selective service. When the meeting in the Auditorium Theater opened, some 4,000 persons listened attentively to the message brought to them by representatives of the Surgeon-General's Office, of the Red Cross and of foreign nations as to the program for the care of the disabled fighting men in order that they may be returned to a useful civilian life. In the meeting on the selective service, Lieutenant-Colonel J. S. Easby Smith and Major Hubert Work, of the Provost Marshal-General's Office, were able to confer with the state aides of the governors of the various states who had been ordered to Chicago for this meeting, and to inform the many physicians representing the 23,000 physicians who are engaged in the work of the local, district appeal, and advisory boards under the selective service law, concerning many points which had not up to this time been made clear to them.

The Thursday night session was again a public, patriotic meeting. The Auditorium was again filled to capacity, the music on this occasion being provided by a detachment of the Great Lakes Naval Training Station Band and by group singing of the audience.

The scientific and commercial exhibits were open throughout the session and were noteworthy for their practical and military features. The commercial exhibit included practically all of the recently developed foods, pharmaceuticals and mechanical devices resulting from necessities created by the advance of scientific knowledge and the military emergency.

Special arrangements were made for entertaining the medical women who attended the session in large numbers and also for the amusement of women guests. The entertainments included receptions, teas, a musical and a visit to the Great Lakes Naval Training Station to attend the dedication of the new Red Cross building. Arrangements had also been made for a visit to the central department Red Cross headquarters and to local merchandise and industrial plants.

To add to the entertainment of the visitors to this session, a medical motion picture show was conducted throughout the session, numerous reels of film lent by the Medical Department of the United States Army being continuously exhibited, including the famous film "Fit to Fight," prepared by the commission on training camp activities. Captain H. M. Strong, post surgeon at Rantoul, Ill., was permitted to visit the annual session in an aeroplane, and arrived promptly on time in Chicago, being greeted by the Fort Riley Band and by many convention visitors.

The meeting also served to inform many physicians who are about to enter the military service concerning the routine of application and appointment, equipment, assignment to duty, etc. Medical military headquarters were in continuous operation, in charge of medical officers on active duty. Over 600 physicians were given information, supplied with application blanks, and many of them sent directly to the local examiner for physical examination.

The attendance at this session was the largest since the Chicago session of 1908. The total, 5,553, is but a meager 800 less than that of the 1908 session, and when one takes into consideration the fact that about 20,000 physicians are in active military service, that the services of many physicians are continuously needed by civilian communities and industrial institutions, and that railroad rates are comparatively higher than they have ever before been in the history of our country, the attendance at this session may well be said to have been phenomenal. No meeting of the Association has so successfully reached the public as did this sixty-ninth annual session, and the public showed its interest in the session and in the work which the medical profession is trying to accomplish, by attending in large

numbers every meeting in which arrangements had been made for it. The local committee on arrangements and the medical profession of Chicago are to be congratulated on the results of this session, and the thanks and appreciation of every Fellow of the American Medical Association is due them.

THE PRODUCTION OF OPTICAL GLASS IN THE UNITED STATES

The War Industries Board authorizes the statement that before the war little effort was made to produce optical glass in the United States. Manufacturers of optical instruments were able to obtain optical glass in desired quantity and quality from Europe and consequently did not feel the necessity for making it themselves. In 1912, however, the Bausch & Lomb Optical Co., of Rochester, N. Y., built an experimental optical-glass plant and placed a practical glassmaker in charge; by 1914 this company was able to produce a few types of optical glass which was used in optical instruments.

By the end of 1914 the importation of optical glass had become difficult and uncertain. Other firms, as Keuffel & Esser, of Hoboken, N. J., and Spencer Lens Co., Buffalo, N. Y., and the Bureau of Standards of the Department of Commerce, at Washington, began to experiment in making optical glass. By 1917, when the United States entered the war, the optical glass situation had become critical. The European supply was practically cut off. Optical glass had to be made in this country if our army and navy were to receive the fire-control instruments which they needed.

The Geophysical Laboratory of the Carnegie Institution of Washington was called upon to aid in the production of high-grade optical glass. A party from the laboratory was stationed at the plant of the Bausch & Lomb Optical Co. in April, 1917, and for seven months all efforts of the laboratory were concentrated at this plant. At the end of 1917 the essential details of the manufacture had been developed and glass in considerable quantities was being produced. The efforts of the laboratory were then extended to the Spencer Lens Co. and to the Pittsburgh Plate Glass Co., Pittsburgh, Pa. During this period the Bureau of Standards rendered effective aid.

At the present time, as a result of cooperation between the manufacturers and scientists, large quantities of optical glass of the kinds needed for military fire-control instruments are being produced of a quality equal in practically every respect to the best European glass. The need for a continuous and assured supply of optical glass is so great that the workmen trained in the details of manufacture and subject to draft, are being withheld from the draft in order that their technical training may be utilized at this time. The required information and details of manufacture and the skill necessary for proper production have been gained at great expense and under high pressure.

THE SOURCE OF TRENCH FEVER[1]

A cablegram from the commanding general of the American Expeditionary Forces to the Secretary of War reports the success of a trench-fever investigation, which was made possible through the willingness of sixty-six American soldiers to risk their lives. The message contains the names and home addresses of the men who submitted to inoculation. All of them now are either cured or convalescent.

These men were from field hospitals and ambulance organizations, units commonly designated as noncombatant. They were selected from a large group of volunteers as the healthiest and consequently the best able to withstand a long siege of trench fever, which has been one of the most baffling diseases which the allied armies have encountered. The men selected were sent to a hospital behind the British front line in January.

Trench fever is a disease which has been common on the western front. It may have existed before, but has not been either frequent or severe enough to direct the attention of the medical profession. Now it represents one of the greatest causes of disability in the allied armies. Nothing definite was known about either the cause or mode of spread of this disease.

[1] Publication authorized by the Secretary of War.

While it is probably never fatal by its nature, through frequent relapses and debilitating effects it may render a certain proportion of men permanently unfit for military service, and the approximate average time lost from this disease is six months. Therefore, in spite of the fact that it is not a fatal disease, from the military point of view it has been a serious one.

The problem of protecting our men, if possible, from this added suffering, was one of the first questions faced by the American Expeditionary Forces. Before any intelligent protective measure could be taken there were two points to be established. First, was this disease caused by germs? Second, if it was a germ disease how was it spread?

Attempts were made to use animals to establish these points, but no animals susceptible to this disease could be found. Therefore, as was the case of Walter Reed and his work on yellow fever, it was necessary to resort to volunteers from our army, who would be willing to sacrifice themselves that the many might be saved.

The first question studied was whether this was a germ disease. No germs could be seen with the microscope, but the Medical Department knew that there are numerous germs which can not be seen by even the most powerful magnification. Therefore this point had to be established by taking blood from men with the fever and injecting it into healthy men. Out of 34 such individuals inoculated with blood or some constituent thereof, taken from 7 cases of trench fever, 23 volunteers developed the disease. Out of 16 healthy men inoculated with whole blood from a trench-fever case 15 developed the disease. These experiments prove that trench fever is a germ disease and that the germs live in the blood of men so infected.

The next question was "How is this disease spread?" Naturally the body louse was to be considered first. Large numbers of these were collected from patients with trench fever and also some of the same kind were brought from England, which had been collected from healthy men. The lice from trench-fever cases were allowed to bite 22 men. Twelve of these later developed the disease, while four men bitten by lice from healthy men remained free from the disease. Eight other volunteers living under exactly the same conditions, in the same wards, but kept free from lice, did not develop trench fever. After blood inoculation the disease developed in from 5 to 20 days. After being bitten by infected lice the fever required from 15 to 35 days to develop.

With these facts in hand, namely, that trench fever is a germ disease and that it is carried by lice, it is now possible to take up the question of controlling, in an intelligent manner, the disease. As long as the protection of the men from lice was only a matter of comfort and of no military importance, their extermination did not warrant extraordinary measures, but now that it is known that it is not simply a matter of discomfort, but that the "cootie" (trench vermin) is incidentally one of the largest causes of disability, it is deemed worthy of extraordinary efforts to control these pests. It is a repetition of the question of mosquito control, yellow fever having been eliminated on the Panama Canal by these means.

It is no mean thing that these volunteers did in France. To face illness of weeks, with extreme suffering, requires peculiar valor. The average loss of weight for these men was from 20 to 25 pounds. Incidentally the hospital in which the experiments were carried out was shelled by the Germans in the early part of their March drive. It is believed by the Army Medical Corps that the sacrifice of this group of 66 men will in time lead to the protection of thousands of men from the ravages of trench fever.

SCIENTIFIC NOTES AND NEWS

AT the commencement exercises of Yale University the degree of doctor of science was conferred on Edward Sylvester Morse, director of the Peabody Museum, and on Dr. Henry Drysdale Dakin, the physiological chemist.

THE honorary degree of A.M. has been conferred by Harvard University on Outram Bangs, curator of mammals, Museum of Com-

parative Zoology at Harvard, and on Hennen Jennings, consulting engineer.

Major Ralph D. Mershon, formerly assistant professor of Ohio State University and now of the Naval Construction Board, has received the degree of D.Sc. from Tufts College.

Williams College has conferred the degree of D.Sc. on Dr. Raymond Dodge, professor of psychology in Wesleyan University.

Bowdoin College has conferred the degree of doctor of science on Charles Clifford Hutchins, professor of physics at Bowdoin; on Donald B. Macmillan, the explorer, and on Colonel Winford H. Smith, the surgeon.

The Willard Gibbs Medal for 1918 was conferred on William M. Burton, Ph.D., in recognition of his distinguished work in petroleum chemistry, at the meeting of the Chicago Section of the American Chemical Society on May 17, 1918. Introductory remarks by L. M. Tolman, chairman of the section, were followed by the presentation of the medal by Dr. Ira Remsen. A reception and dinner preceded the meeting at which informal addresses were made by Lucius Peter, president of the Chicago Association of Commerce; Thomas F. Holgate, president of Northwestern University; George N. Carman, president of Lewis Institute; W. E. Stone, president of Purdue University, and Julius Stieglitz, director of the department of chemistry, University of Chicago.

Dr. Allen Rogers has been appointed a major in the chemical service section of the National Army. He will be in charge of the Industrial Relations Department.

Dr. E. B. Forbes, head of the department of nutrition of the Ohio Experiment Station, has been commissioned a major in the Food Division, Sanitary Corps.

Dr. A. E. Kennelly, acting head of the department of electrical engineering at the Massachusetts Institute of Technology in place of Professor D. C. Jackson, who went into government service a month ago, has been called to Washington for special work with the Signal Corps. He will be away from the institute during the summer months, but expects to return in the fall.

The Pereira medal of the Pharmaceutical Society of Great Britain has been awarded to Miss H. C. M. Winch.

Dr. Norman Walker has been appointed inspector of anatomy for Scotland in the room of the late Sir James A. Russell.

G. Montague Butler, E.M. (Colorado School of Mines), has been appointed director of the Arizona State Bureau of Mines to fill the vacancy created by the resignation of C. F. Willis. He will continue to serve as dean of the College of Mines and Engineering, which position he has held for three years. The new director of the bureau intends to lay greater emphasis upon geological investigations, and will collect the data required for the preparation of a reconnaissance geological map of Arizona.

Professor Arthur Harmount Graves, formerly of Yale University, has been appointed by the Office of Forest Pathology, Bureau of Plant Industry, for work during the summer months on problems relating to disease resistance in the chestnut tree.

Professor Frank T. McFarland, of the department of botany of the University of Kentucky, has charge for the summer of the white pine blister rust eradication in the states of Kentucky, Tennessee and Missouri, with headquarters at Lexington, Ky.

Dr. S. K. Loy has resigned his position as professor of chemistry at the University of Wyoming to become chief chemist for the Midwest Refining Company. His office will be at Casper, Wyoming.

Mr. W. J. McGee, of the Bureau of Chemistry, U. S. Department of Agriculture, and formerly stationed at Savannah, Ga., has been transferred to San Juan, Porto Rico, where he is engaged in the inspection of food and drugs.

The Croonian Lecture before the Royal Society was delivered by Major W. B. Cannon, professor of physiology, Harvard Medical School, on June 20, the subject being "The physiological basis of thirst."

JOHN HARPER LONG, professor of chemistry at the Northwestern University Medical School for thirty-seven years died at his home in Evanston, Ill., on June 14, aged sixty-two years. Dr. Long, distinguished for his work in physiological chemistry, was vice-president of the American Assocation for the Advancement of Science in 1901 and president of the American Chemical Society in 1903.

FRANK N. MEYER, of the Department of Agriculture, has died in China. Mr. Meyer had travelled as an agricultural explorer through China, Siberia and Turkestan for nearly ten years and had introduced here many species and varieties of plants. He discovered the home of the chestnut bark disease.

MAJOR EUGENE WILSON CALDWELL, of the Medical Reserve of the army, an X-ray expert who recently perfected a device for stereoscopic fluoroscopy, died last week as the result of an operation to remove a cancerous formation on one of his arms, caused by burns received some months ago in making X-ray experiments.

A CABLE from London to the daily papers states that the American Army, at the suggestion of the French, is adopting the metric system for all war purposes, *e. g.,* for artillery, machine-guns, maps, etc. The convenience of such an arrangement is obvious as all "parts" become thereby interchangeable.

UNIVERSITY AND EDUCATIONAL NEWS

GIFTS to Yale University in the past year and credited as endowment made a total of $1,279,764, the alumni were informed by President Arthur T. Hadley at the commencement luncheon. From time to time gifts have been announced, but the new items included $100,000 as the Earl Williams Fund from Mrs. James Harvey Williams for the benefit of the University Press, and $400,000 from William L. Harkness, '81, as a building fund. The Williams Fund is a memorial to Earl Williams, 1910, 301st Field Artillery, who died in May. For the present the income will be used in war relief. The Harkness building after the war will be placed on Dwight Hall site, and will contain lecture and classrooms.

UNDER a compromise agreement Columbia University will receive half the estate of the late Robert B. Van Cortlandt. The value of the estate is said to be about $1,000,000.

A GIFT of Liberty Bonds and checks totalling $100,000 to Harvard University from 206 members of the class of 1893 is announced.

EIGHTEEN fellowships and thirty-three scholarships have been established for students in chemistry at colleges and universities throughout the country by the du Pont Company. The total value of these awards will be $25,000, the fellowships carrying $750 and the scholarships $350 each for the coming scholastic year. The fellowships are distributed among seventeen colleges and universities and the scholarships go to thirty-one institutions of learning scattered through the country, every section from the Atlantic to the Pacific, and from the Canadian border to Texas being included. The fellowships are for postgraduate work and will be established in the institutions which have the most advanced courses in chemistry. The scholarships go to members of the senior classes in institutions which pay particular attention to chemical instruction. The recipients of these awards, which are to be known as "du Pont fellowships" and "du Pont scholarships," are to be selected by the institutions themselves, the only condition made by the du Pont Company being that they shall go to students who have devoted the major part of their time to chemistry.

THE *Journal* of the American Medical Association states that a donation of $2,000,000 has been promised to the University of Toronto for research work. Professor A. B. Macallum, chairman of the scientific and industrial research council of Canada, urges that research science faculties should at once be established at McGill and Toronto universities.

DR. LAUDER W. JONES, head of the department of chemistry in the University of Cincinnati, has resigned to become head of the department of chemistry in the University of

Minnesota. He has been granted a leave of absence for a year in order to take charge of the Research Division of the Gas Offensive at the American University in Washington. Dr. Harry S. Fry, associate professor, has been appointed acting head of the department of chemistry in the University of Cincinnati.

John F. Guberlet, A.M. ('11, Illinois), Ph.D., '14 (zoology), who since 1915 has been professor of biology at Carroll College, Waukesha, Wisconsin, has recently accepted the position of assistant parasitologist at the Oklahoma Agricultural and Mechanical College and Experiment Station, at Stillwater, Oklahoma. He will take up his work in Oklahoma on July first.

Herbert Ruckes, in charge of the department of biology at Grove City College, has resigned to accept a position in the department of biology at the Agricultural and Mechanical College of Texas. For the past year Mr. Ruckes has been carrying on a botanical survey of Mercer county, Pa.

Professor H. V. Tartar, who for the first five years has been station chemist and associate professor of agricultural chemistry at the Oregon Agricultural College, has accepted a position in the department of chemistry of the University of Washington at Seattle.

DISCUSSION AND CORRESPONDENCE

SOLUTION TENSION AND INDUCTIVITY

To the Editor of Science: In Science of May 3, Professor Fernando Sanford, of Stanford University, describes a concentration cell in which the direction of deposition is the reverse of what would be expected if it were previously assumed that the solution tension of the metal is constant for both solvents. He offers an explanation connecting the phenomenon with the dielectric property of the solvent.

In the absence of quantitative data, the great difference known to exist between the solution tensions of a metal in different solvents would seem a sufficient explanation. It is true that in the Nernst theory of the concentration cell prior to 1894 it was supposed that the solution tension of a metal was a constant property of the metal at a given temperature; but the supposition was short lived, as it involved a difficulty exactly like the one in question, and led to measurements of solution tension in water and in alcohol,[1] so that apparently a difficulty has been raised which does not exist.

It may well be, as Professor Sanford suggests, that there is a relation between solution tension and the inductivity of the solvent, just as there must be a relation between inductivity and dissociating power, since the forces between charged bodies vary inversely as was remarked by J. J. Thomson and by Nernst. The same consideration would indicate a relation between the effective solution pressure of a metal and inductivity, since there could hardly be a more typical condenser than the Helmholtz "double layer." Certainly the quantitative investigation of the matter is greatly to be desired.

An assumption of constancy of solution tension of a metal in contact with varying concentrations of its ions in the *same solvent* is not warranted; although the results of computations using the equation for electromotive force,

$$\pi = \frac{RT}{nF}\left(ln\frac{P_1 \cdot p_2}{P_2 \cdot p_1}\right),$$

in which the solution tensions, P_1 and P_2, are assumed to cancel, and the ionic concentrations, $m_1\alpha_1$ and $m_2\alpha_2$, are substituted for the osmotic pressures, p_1 and p_2, would indicate that the simplified equation is at least approximately true.

On *a priori* grounds, the assumption is contradicted by the probability that the maintenance of ionization is largely due to an association of the charged particle with molecules of the already associated solvent, as well as that large inductivity and association certainly accompany each other, even if no simple relationship exists. So that it seems reasonable to expect, as he points out, that the inductivity of a solvent would change with changing concentration of ionic solute. But the change is in

[1] H. C. Jones, *Zeitschr. f. physik. Chem.*, 14, 346 (1894).

the opposite direction to that supposed in Professor Sanford's explanation, because increase in ion content must increase the inductivity of the solution, as will appear from the following consideration:

As ions pass from the metal into the solution, the changing composition of the mixture is accompanied by an increase in its density. The density, d, of a solution of any given concentration is related to its index of refraction of light, n, approximately as shown by the equation, $(n-1)/d = R$, the specific refractive power, a constant. A more concentrated solution, *i. e.*, a different proportion of the same components, which has a greater value for d, will also have a greater value for n, since the values of these physical properties depend additively upon the values of the same properties of the components. It would not be proper to substitute for n in the above expression the square root of the dielectric constant, as the electromagnetic theory might suggest, because the latter relationship is not capable of experimental test under the conditions for which the former is found to hold. But while the exact form of the function may be unknown, there can be no doubt that when refractive index increases as in the above case, the inductivity must increase also.

Applying this to a concentration cell, on the dilute side the inductivity of the solution is increasing, and this increment in the inductivity favors the further solution of the metal, but the osmotic pressure of the metallic ions is also increasing, and this increment opposes the further solution of the metal. Solution pressure, the predominating force on the dilute side, is aided by inductivity, and these together constitute a growing force—opposed, however, by a more rapidly growing force, osmotic pressure. In the more concentrated solution around the other electrode, we have an initially greater inductivity which is decreasing as metal ions are discharged and deposited, and this decrease of inductivity favors the deposition (or opposes the solution) of the metal; but the osmotic pressure of the metallic ions is decreasing also, and this decrease opposes the deposition. On this side, solution pressure is aided by a relatively large but decreasing inductivity, and combined they constitute a diminishing force which is initially weaker than the opposing osmotic pressure, but stronger than the corresponding solution pressure of the other electrode. All of the pressure differences in the cell owe their existence to the difference in concentrations of the solutions, and all reach equilibrium when the concentrations become equal.

In formulating the total combined effects on both sides of the cell the inductivity effect is either added to the solution pressure or subtracted from the osmotic pressure of the cations in solution. We are not so much concerned here with the value of the ratio we call inductivity or its nature, as with its effect, which is a pressure. Let us call this the modulus i, then the familiar equation becomes

$$\pi = \pi_1 - \pi_2 = \frac{RT}{nF}\left(ln\frac{P(1+i_1)}{p_1}\right) - \frac{RT}{nF}\left(ln\frac{P(1+i_2)}{p_2}\right) = \frac{RT}{nF}\left(ln\frac{p_2(1+i_1)}{p_1(1+i_2)}\right).$$

In this we have assumed, after all, that fundamentally the solution pressure is constant, but that there is a difference in *effective* solution pressure due to difference in inductivity. This seems reasonable where we are dealing with the same solvent as in a simple concentration cell: here the differences in inductivity are probably small. Would this equation suffice for different solvents in which i_1 and i_2 are unrelated, or must we still keep P_1 and P_2 distinct and find some further cause for a difference in solution tension of the same metal?

A series of inductivity measurements for varying concentrations of, say zinc sulphate, in water, with measurements of electromotive force of elements composed of zinc in the same concentrations of the salt, might lead to a clearer knowledge of the magnitude of solution tension, and might even throw some light on the as yet unknown forces whose resultant we call dissociating power.

In conclusion, allow me to say that this is not written in a spirit of controversy, but in order to place a little of our existing knowl-

edge at what may be a new angle to some one else who may thereby perceive a generalization or means to discover one. This, which seems to be the purpose of discussion, will be served as well even though I may have fallen into errors far more grievous than the apparent one that has occasioned this communication.

Horatio Hughes

THE TRUE SOIL SOLUTION

Just recently, Dr. C. B. Lipman has published[1] a preliminary paper describing a "new method of extracting the soil solution," by subjecting the soil to a maximum direct pressure of 53,000 pounds to the square inch. This preliminary article describes briefly the apparatus used in obtaining this enormous pressure and claims for this new method the "obtaining of the soil solution *as it exists*[2] in relatively thin films around the soil particles. The procedure is rapid, clean and of high efficiency. With further improvements in apparatus which we now are planning, the method should supplant all other methods known to-day, including even the Morgan procedure." The fault found with the Morgan method is that it is "laborious and slow, and introduces the factor of oil which complicates and renders it extremely time-consuming and untidy."

Let us look at the important points Dr. Lipman claims for his direct-pressure method.

> It allows of the direct determination of the concentration of the soil solution, and of the amounts of each of the solutes contained therein.

The physical chemist is familiar with the fact that pressure is a considerable factor in influencing solubilities and it does not seem logical that a method employing such enormous pressures could obtain the soil solution "as it exists" in the soil without upsetting the whole physico-chemical equilibrium of the real soil solution; its specific gravity, viscosity, surface tension, osmotic pressure, specific conductivity and its chemical composition would all suffer more or less of a change which would combine to render the solution worthless to the plant physiologist or to the plant physiological pathologist from a scientific point of view. The reason that the soil solutions obtained by other methods are at fault is largely because the water added in extracting the soil changes the solubilities of certain of the ingredients. The van Suchtelen-Itano paraffin-oil displacement-pressure method described by Morgan[3] was worked out carefully with just the opposite idea in mind, *i. e.,* to subject the soil to *as little pressure as possible* so as to preserve intact the physico-chemical equilibrium of the solution obtained. To this end the most inert oil was carefully selected as the displacement medium and pressures not exceeding 500 pounds per square inch were employed for forcing the oil into the soil. The preliminary tests[4] of the paraffin-oil displacement-pressure method, run by van Suchtelen and Itano before extensive work was done by these investigators and by Morgan, show that the inactive paraffin oil when brought into intimate contact with the soil solution did not change the electrical conductivity, chemical composition nor surface tension. The solution is literally pushed out of the soil by the inert oil, only sufficient pressure being used to force the viscous oil into the soil.

The oil-pressure method is somewhat time-consuming, laborious and untidy, but common workmen after being carefully instructed can do this work under the supervision of the trained scientists; again, not one but a battery of as many cylinders as desired can be used to obtain sufficient quantities of solution in a minimum time. However if Dr. Lipman's above contentions did hold true in every respect the end in view, *i. e.,* the obtaining of a solution representing *most nearly in all re-*

[1] Lipman, C. B., "A New Method of Extracting the Soil Solution," Univ. of Calif. Publ. in Agr. Sciences, Vol. 3, No. 7, pp. 131–134, March 15, 1918.

[2] Italics ours.

[3] Morgan, J. F., "The Soil Solution Obtained by the Oil Pressure Method," *Soil Science,* Vol. II., No. 6, 1917, pp. 531–545, Pl. 1.

[4] Report of the Bacteriologist, 26th Annual Report of the Michigan State Board of Agriculture, pp. 152–153.

spects that of the actual soil solution, should be the first consideration.

To the soil bacteriologist the solution obtained under great pressures would be of doubtful value. Many bacteria are destroyed by high pressures (25,000 to 100,000 pounds). In fact high pressures alone have been employed successfully in the sterilization of fruits and vegetables.[5] Studies of the microorganisms surviving these enormous pressures would be probably only a matter of curiosity and of no immediate value or utility.

It seems that Dr. Lipman should have made a thorough comparative study of the soil solution obtained from the same soils by the two methods under discussion before he could be justified in making the statements set forth in his preliminary article.

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DRAWINGS ON LANTERN SLIDES

PROFESSOR GUNTHORP'S letter in SCIENCE for April 12 in regard to drawings on lantern slides seemed to the writer to be an attempt to solve the problem of writing upon clear glass when the ordinary coated slides were not available. The letter by Mr. Benton in the issue for May 17 goes further into the solution of this problem, and the suggestion of using india ink is a good one, but the idea of pasting paper to a slide to secure a purchase for the foot of a compass would lead one to suspect that the use of coated glass had not been tried. Even though this supposition is in error the use of ordinary unexposed lantern slides, fixed in the dark room, or of old slides reduced by successive immersions in "hypo" and Farmer's solution, may be new to some and is worthy of mention. The transparency of the prepared slide is all but perfect, the coated side can be written, drawn or ruled upon at will, areas can be shaded or colored, errors can be removed by the simple expedient of scratching away the gelatin (and the remaining scar is surprisingly insignificant when the slide is thrown upon the screen) and dividers or compass can be used without danger of slipping. In writing, the finer and firmer the point, and the less ink, the better, as a thick line will crack up into a mosaic; and experience has shown that ordinary fountain pen ink is much less liable than india ink to crack in this way or to "ball" at the ends of the strokes. Waterproof inks must be used, however, if the slides are to be wet. If it is desired to render large areas opaque, and it is impracticable to use successive thin coatings, cover them with india ink, preferably using a brush, and when the surfaces have dried and cracked cover them again.

In coloring slides drawing inks may be used and the surfaces so colored will not crack if the ink is applied in thin enough coats. Higgins's carmine will be found less suitable than the other reds because of its heaviness of body and rapidity of drying. A simple method of improving one's chances of securing a smooth result, however, is to soak the drafted slide in water and then allow it to dry until there is no free moisture present, until it is sticky, before the colors are applied. If these precautions are taken and the wash is not too thick an even uncracked surface will result. Water colors, especially the stains and "lakes," are highly transparent and generally preferable to many of the drawing inks. For blended outlines the colors should be put on while the slides are covered with water in the customary way, but for the sharp outlines which will usually be desired in drafted slides the latter should be approximately or entirely dry.

Lantern slides prepared in this way need not be covered to preserve the writings or figures from abrasion, always a troublesome feature when clear glass is used. Fingermarks will show, though a slide pinched between the fingers will take a mark more readily upon the clear than upon the coated side, but these can be removed from the latter, almost irrespective of the ink or coloring materials used, by washing with pure alcohol. The ounce of pre-

[5] Hite, B. H., Giddings, N. J., and Weakley, Chas. E., Jr., "The Effect of Pressure on Certain Microorganisms Encountered in the Preservation of Fruits and Vegetables," West Virginia Station Bulletin 146, 1914.

vention is to paste a border of ordinary black binding tape on the coated side before work upon the slide is begun. This is an easy method also of inhibiting any tendency to write upon more of the slide than can be shown upon the ordinary screen.

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CELLOIDIN-PARAFFIN METHODS

THE review of Apáthy's[1] celloidin-paraffin method published in SCIENCE by S. I. Kornwork the present writer chanced upon, although actively interested between the years 1912–1915 in similar methods of imbedding plant tissues.

During research studies in plant anatomy, bulbs of *Cooperia Drummondii* were found to be particularly troublesome material to imbed. The delicate scales contain starch, calcium oxalate crystals and a mucilaginous slime which may coagulate during killing and fixation. These scales are attached to a base or axis formed of parenchyma, it is true, yet of parenchyma of an entirely different structure from that of the scales. The difficulties encountered because of the included materials plus the variance in structure of the bulb axis and its attached scales caused a wide search for a suitable imbedding medium. The choice at last was a combination of celloidin and paraffin, the advantageous qualities of which can not be emphasized too strongly. As Dr. Kornhauser points out, celloidin in contact with the object prevents shrinkage of the material on cooling and paraffin allows of serial sections which can be readily spread on the slide. Whether there are advantages or disadvantages in Apáthy's oil mixture I do not know, having never tried it, but I do know that entirely satisfactory results can be obtained with material which can be handled neither in paraffin, in celloidin, in agar-agar, nor in rubber and paraffin, by a much more simple celloidin-paraffin method than that of Apáthy's. The technique planned and followed out by the present writer was simply as follows: Material is treated to the celloidin process of imbedding up to the point where the object would usually be set in a block. Instead all surplus celloidin is removed from the object which with the adhering and infiltrated celloidin is hardened in 70 per cent. alcohol and later placed for clearing in chloroform for two hours. The next step is to place the object in 85 per cent. alcohol and from there on to follow the paraffin method. Material thus treated cut with an unusual smoothness, making it possible to obtain serial sections 10 μ in thickness with an ease that was a surprise and also a great comfort.

If one desires to cut serial sections of objects too large for the block of a rotary microtome or to be handled in paraffin, such large objects imbedded in celloidin (mature bulbs) can be cut into sections 50–75 μ thick with the sliding microtome, and placed immediately in 70 per cent. alcohol, from which they can be carried through the alcohols and imbedded in paraffin. It seems probable that Apáthy's oil mixture would be a valuable asset here because in cases where it is necessary to retain considerable celloidin, *e. g.*, in handling bulbs where the scales ordinarily fall apart on cutting, it would prevent the shrinkage caused by the drying effect of the alcohols and the heat from the bath.

There are surely two advantages to the celloidin-paraffin method as commonly used by the writer, (1) its simplicity and (2) the removal of surplus celloidin, a substance affected by the drying effect of the higher alcohols and heat and also inert itself in histological value and yet troublesome because of its affinity greater than that of plant tissues for stains such as gentian violet and safranin.

MARGARET B. CHURCH

[1] Apáthy, S., 1912, "Neuere Beitraege zur Schneidetechnick," *Zeitschr. wiss. Mikr.*, Bd. XXIX., S. 449–515, 4 textfiguren.

[2] Kornhauser, S. I., "Celloidin Paraffin Method," SCIENCE, N. S., Vol. XLIV., No. 1134, pp. 57–58, July 14, 1916.

ALLIGATORS AS FOOD

AN article by the writer on "Reptiles as Food," which appeared in the December, 1917, number of *The Scientific Monthly*, having

excited the curiosity of the members of a large boarding house in Morgantown, W. Va., it was suggested that a collection be taken among those interested to buy a couple of small alligators and have them cooked, to see if the flesh really was as agreeable as was claimed in the article in question. The writer agreed to buy the animals and prepare the flesh for cooking.

Sufficient funds were collected to buy, of the Arkansas Alligator Farm, two alligators, each about three feet in length. These were killed by cutting the cord at the base of the skull, and the flesh of the entire body was cut into pieces of suitable size for cooking.

The meat was first parboiled (though the necessity for this was doubtful) and was then fried in egg and cracker crumbs, very much after the manner of a breaded veal cutlet.

About thirty people, consisting of both men and women, mostly school teachers, members of the university faculty, and college students, partook of the repast, and all declared the meat to be "delicious."

There was considerable difference of opinion as to what the meat resembled: some thought it tasted like pork; some thought it like fish; one person said it suggested lobster; but all declared it to be most agreeable.

Of course, at the prices charged by supply firms the cost of live alligators would be prohibitive, but in the tropics, where crocodilia are often extremely abundant, the flesh could be had at a very low cost.

The writer has seen alligator hunters, in our Southern states, throw hundreds of pounds of alligator meat to the carrion crows and buzzards, after removing the hides.

Whether the Central and South American crocodiles would be as pleasant for food as the Florida alligator the writer can not say.

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SCIENTIFIC BOOKS

Genetics in Relation to Agriculture. By E. B. BABCOCK and R. E. CLAUSEN. New York, McGraw-Hill Book Co. 1918. Pp. xx + 675, 4 plates.

PROFESSORS BABCOCK and Clausen have given us a valuable new book on the subject of genetics. As the title suggests, the book is intended primarily as a text-book for students of agricultural genetics. It will, however, be of great value also to those whose interest in genetics is not primarily in its agricultural bearings. This is true partly because of the very fact that the authors have brought together a large amount of data from the agricultural publications that is not ordinarily familiar to the geneticist who is working along other lines.

The book is divided into three parts, entitled Fundamentals, Plant Breeding and Animal Breeding. All three contain much material that will be interesting to all students of the subject.

In part 1 the advanced student will find little that is new to him; but he will find a clear and well-written account of the important principles of genetics. The material drawn on for purposes of illustration is well selected, and is up to date—a very important point in a subject developing as rapidly as genetics. In the case of matters still under debate, such as multiple allelomorphism and selection, the authors have presented both sides of the question impartially, and have then weighed the evidence and drawn their own conclusions as to probable correctness. The chromosome hypothesis is adopted, and is used throughout the book in interpreting examples. The work on *Drosophila* is given a prominent place, and the results obtained with that fly bearing on the questions of linkage, crossing over, non-disjunction, mutation, multiple allelomorphs, etc., are carefully and simply presented. The question of pure lines and selection is discussed at some length, and the conclusion is reached that multiple factors offer the most plausible explanation of the phenomena.

The chapters on species hybridization and on the statistical study of variation should both be useful, as they present material that is not adequately discussed in other standard text-books on genetics in English. In the latter chapter the standard deviation and average product-moment are referred to as

absolute values, the coefficients of variability and of correlation as relative values. It seems to the reviewer that the latter, rather than the former, are absolute values; for they are the ones that are independent of the units of measurement used. The point is not one that is likely to cause confusion, as it is at all times clear what the authors mean.

In the chapter on species hybridization the "reaction system" idea is discussed at some length. According to this hypothesis, which has been developed by Goodspeed and Clausen on the basis of their species crosses with tobacco plants, the whole group of genetic materials of a species (the "reaction system" of that species) may behave as a single unit, or nearly so, in influencing dominance and viability in hybrids. The term "reaction system" seems to the reviewer to be rather unfortunate, as it is commonly applied to any system that is undergoing change, organic or otherwise. There can be no question of the very great interest of the facts that have been discovered by Goodspeed and Clausen, but in the opinion of the reviewer more detailed evidence is needed, especially as regards the cytological behavior of the F_1 hybrids, and the genetic behavior of the plants produced by back crossing the F_1 to both parent species, before the conception can be adopted as more than an interesting suggestion.

In parts 2 and 3 a large amount of data bearing on the genetics of domestic animals and plants has been brought together, and has been presented in a thoroughly scientific manner. This makes these sections useful also to the non-agricultural geneticist. To the practical breeder these sections should be invaluable, not alone because of the genetic data they contain, but also because of the discussions of methods of securing and recording information, and of the practical application of genetic knowledge. The chapter on beliefs of practical breeders is especially noteworthy; it gives in concise and convincing form the evidence against telegony, maternal impressions, and similar notions sometimes held by breeders.

The subject of eugenics is treated only very briefly and incidentally, and even then with a word of warning as to the reliability of the conclusions reached. No attempt is made to take advantage of the great popular interest in eugenics by exaggerating the importance and significance of the results that have been reported.

The authors are to be congratulated on a book that is well printed, well illustrated, well written, and that contains a surprisingly large amount of material that is conveniently arranged and adequately presented.

A. H. STURTEVANT

SPECIAL ARTICLES

A NEW GRAPHICAL METHOD FOR COMPARING PERFORMANCE WITH PROGRAM OR EXPECTATION

THE graphic method is generally recognized as a most important means to interpret facts to administrative executives as well as to the public. For the sake of simple comparison the well-known bar diagram (besides many other diagrams) is, of course, frequently used, but the latter, especially in the case of a variable delivery or production against time and a fixed quantity (requirements or expected production), loses its value. This characteristic rigidity of the bar diagram permits analysis of only one particular instant of the situation, with no reference to the past or future. For instance, if the total output of flour of a certain milling division starting from January 1, 1918, up to, let us say February 9, 1918, is expected to be 1,314,000 barrels, and up to

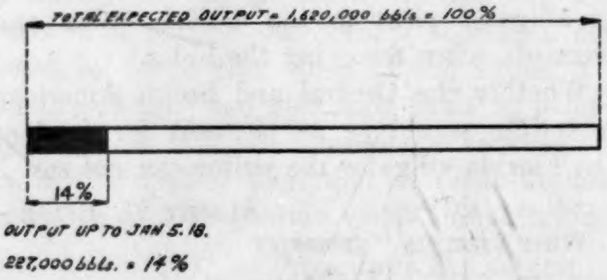

FIG. 1.

January 5, 227,000 barrels of flour have been manufactured, the situation expressed by a bar diagram (Fig. 1), using the expected output as a 100 per cent. basis, would be:

Total expected output = 1,620,000 barrels = 100 per cent.

Output up to January 5, 1918 = 227,000 barrels = 14 per cent.

We can see readily that no conclusions whatever could be drawn as to the output rate or as to the actual number of barrels of flour

The expected output after having been carefully calculated is based on a uniform production rate throughout the given time. Graphically expressed the requirements or the expected output are a straight line, the tangent of which is the production rate or as shown in Fig. 2.

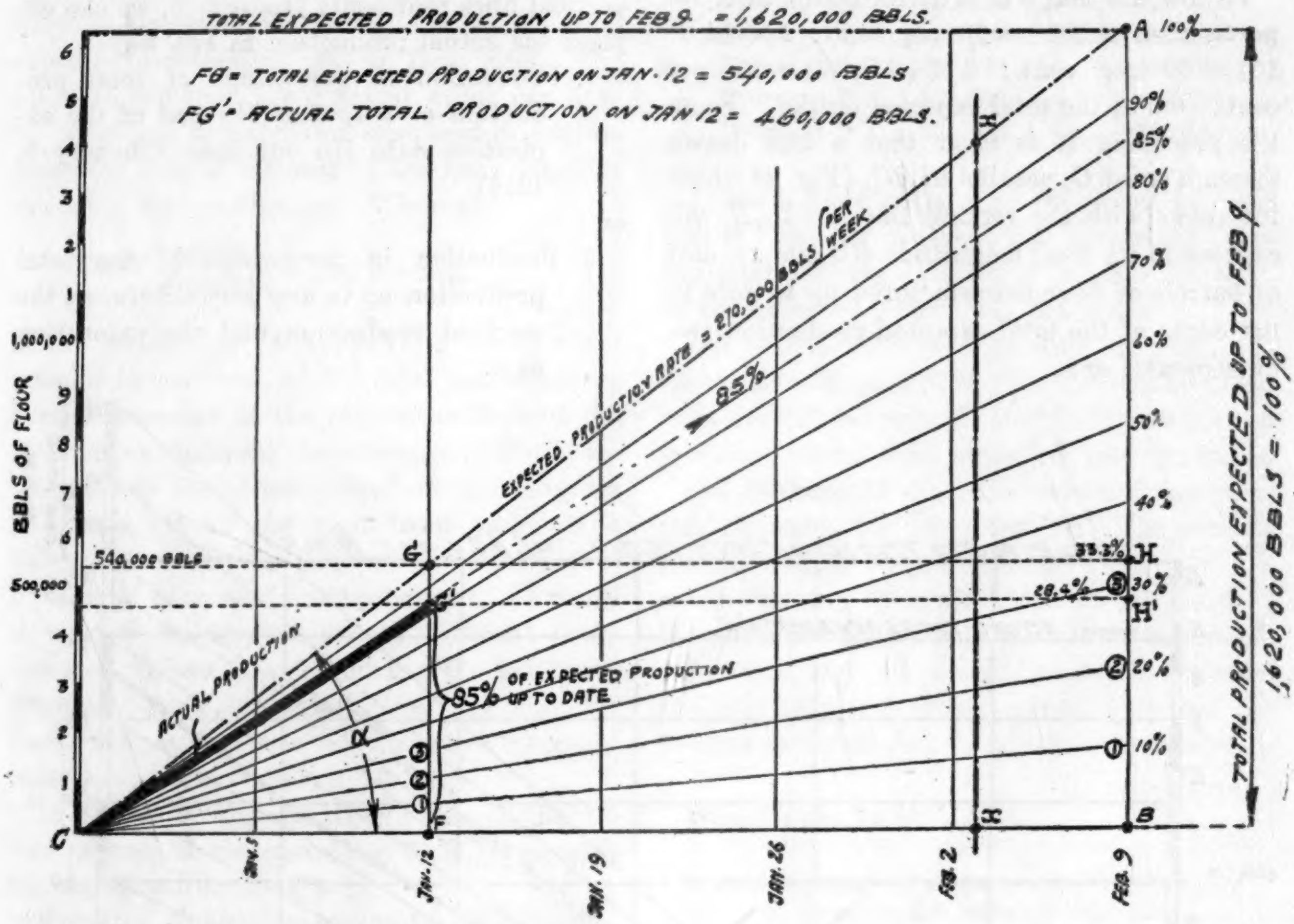

FIG. 2.

which should have been manufactured up to January 5, 1918, if the goal is to be reached on February 9, 1918. Besides that the bar diagram requires each time, when the output has been increased, a new calculation of the output in percentage of the total expected output.

To overcome this difficulty and especially to avoid the constant recalculation and redrawing of the bars a more efficient graphic method was devised. The new method with its new diagram automatically takes care of the whole period in which production is expected.

$$tg\alpha = \frac{\text{Total expected production}}{\text{Total time}} = \frac{AB}{CB}$$

$$= \frac{1{,}620{,}000}{6} = 270{,}000 \text{ bbls. per week.}$$

Based on this average uniform production rate the total required production at the end of the sixth week (February 9, 1918) would be

$$270{,}000 \times 6 = 1{,}620{,}000 \text{ barrels.}$$

For any time interval the total expected production can be easily deducted from the following proportion (Fig. 2):

$$CB : BA = CF : FG,$$

FG being x,

hence

$$x = \frac{BA \times CF}{CB} \text{ or in figures,}$$

$$x = \frac{1,620,000 \times 2}{6} = 540,000 \text{ barrels,}$$

which represents the required production of the milling division for a two-weeks period.

If now the line AB is divided into 10 equal parts, then $AB/10 = 10$ per cent.; $2 \times (AB/10) = 20$ per cent.; $3 \times (AB/10) = 30$ per cent., etc., of the total expected output. From the preceding it is clear that a line drawn through point G, parallel to BC, (Fig. 2) which intersects with the vertical line AB in H will express in its total magnitude BH the amount of barrels of flour manufactured *up to date* in per cent., of the total expected production (requirements) or:

$$\frac{540,000}{1,620,000} = 33.3 \text{ per cent.}$$

If furthermore FG is divided into ten equal parts and FG–1 is connected with AB–1, and FG–2 is connected with AB–2, it is easily seen that all vertical lines between C and B, for instance X—X, are also divided into 10 equal parts, and as the magnitude of all vertical lines represents production, we can express the actual production in two ways:

1. Production in *percentage of total production or output at the end* of the expiration date (in our case February 9, 1918),

or

2. Production in percentage of the total production up to *any period between* the start of production and the expiration date.

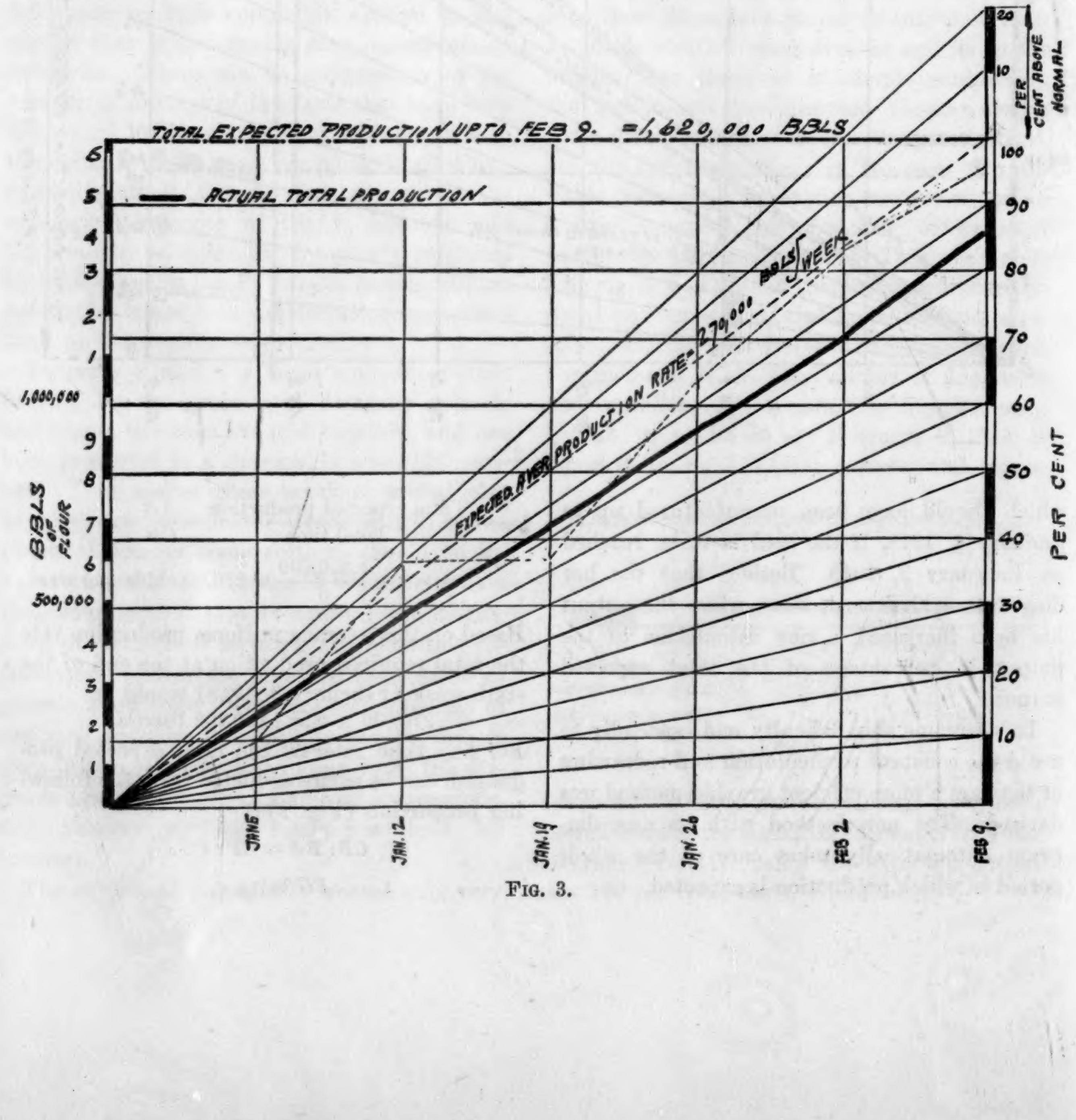

FIG. 3.

To illustrate that; if for instance the total production of a milling division up to January 12 should be 460,000 barrels instead of the expected 540,000 barrels production up to January 12, 1918, equals the magnitude of

$$FG^1 = \frac{460,000}{540,000} = 85 \text{ per cent. (Fig 2)},$$

which is the total actual production up to January 12 expressed in percentage of the total expected production up to *that date.* The other 15 per cent. are a *deficit* which can only be equalized through an *accelerated production rate* during the rest of the time which is available for production. Whereas:

$$\frac{460,000}{1,620,000} = 28.4 \text{ per cent.}$$

is the production of the milling division expressed in per cent. of *the total* production in barrels expected at the *expiration date* of the total time interval in question. Both instances are easily recognized in the diagram and read off on the right-hand scale. The left-hand scale in the diagram gives the *actual* number of barrels of flour *produced.* Through this graphical method all the different functions which are at work during the total production period are easily analyzed and are thus made available to the executive for rapid information and decision.

In the following one simple case of the design of such a diagram (Fig. 3) is given with the statistical data. Given:

Expected Uniform Average Weekly Production Rate: 270,000 barrels.

Time: 6 weeks.

Total Expected Production at the End of Sixth Week: 1,620,000 barrels.

ACTUAL PRODUCTION, PER WEEK ENDING

Actual Production During the Week Ending in Barrels		Cumulative Barrels
January 5	227,000	227,000
January 12	233,000	460,000
January 19	211,000	671,000
January 26	237,000	908,000
February 2	231,000	1,139,000
February 9	246,000	1,385,000

The actual production line shows that the production during the entire period has been about 15 per cent. behind the expected output. Suppose, to show the universality of the diagram, production would have been according to the next table:

Actual Production During the Week Ending in Barrels		Cumulative Barrels
January 5	170,000	170,000
January 12	430,000	600,000
January 19	No production. Strike	600,000
January 26	430,000	1,030,000
February 2	320,000	1,350,000
February 9	200,000	1,550,000

If these facts are plotted (as shown by light dotted line) the diagram will give the following analysis: On *January 5* only 60 per cent. of the expected output was produced.

On *January 12* the production rate was above normal and therefore the intersection with the "above normal line" indicates that 10 per cent. more than expected was produced.

On *January 19* the mills were not in operation on account of labor trouble. The parallel line indicating no production.

On *January 26* again about 95 per cent. of the expected output up to date is produced.

In this way all actual production phases against time and requirements, expected deliveries or needs can be graphically analyzed.

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THE SPECIFIC CONDUCTIVITY OF WATER EXTRACTS OF WHEAT FLOUR[1]

THE attention of cereal chemists has long been attracted to the development of methods for determining the grade of flour and meals. As early as 1884 Girard[2] suggested a procedure for estimating the proportion of fibrous structures in a cubic millimeter of the material. Vidrödi[3] (1893) called attention to the close parallelism between the grade of Hungarian wheat flours and the percentage of ash which

[1] Published with the approval of the Director as Paper No. 123 Journal Series Minnesota Agricultural Experiment Station.

[2] Girard, A., *Ann. d. Chim. et de Phys.*, 6 ser., 3, 293, 1884.

[3] Vidrödi, *Ztschr. angew. Chem.*, 1893, 691.

they contained. Various other tests have been proposed for this purpose, and the most successful may be summarized as follows:

(a) The percentage of ash.

(b) The relative proportion of fiber and debris, as suggested by Girard[2] (1884), Buchwald[4] (1913), and others.

(c) The content of pentosans, as indicated by Koning and Mooj[5] (1914).

(d) The relative change in color resulting from the addition of water and subsequent exposure to the air, a procedure making use of this color change having been patented by Hein[6] (1910).

(e) The catalase activity, as developed by Wender and Lewin[7] (1904), Miller[8] (1909), and others, and discussed in a paper by the writer[9] (1918).

(f) The comparative color of the dry flour, particularly after compression on a slab, the procedure being commonly known as the Pekar test.

The percentage of acidity, and of the soluble proteins and carbohydrates ordinarily diminishes with increasing refinement and "grade" of wheat flour. There are many exceptions to this rule, however, due to unsoundness of the wheat, or decomposition of certain flour constituents subsequent to milling, which may result in increasing the percentage of one or all of these groups of substances. The ash content is apparently more generally employed for detecting the relative grade of wheat flour than any other tests mentioned, with the possible exception of color. The latter is objectionable as a test because of the difficulty of expressing the results numerically. While certain instruments have been suggested for this purpose their use is not common.

[4] Buchwald, J., *Ztschr. ges. Getreidew*, 5, 50, 1913.

[5] Koning, C. J., and Mooj, W. C., Jr., *Chem. Weekblad*, 11, 1064–66, 1914.

[6] Hein, G., German Patent 250,413, November 13, 1910.

[7] Wender, N., and Lewin, D., *Oester. Chem. Z.*, 7, 173–175, 1904.

[8] Miller, M., *Ztschr. ges. Getreidew.*, 1, 194–200; 214–222; 238–244, 1909.

[9] Bailey, C. H., *Jour. Biol. Chem.*, 32, 539–545,

The almost universal use of the ash content in this connection suggested to the author that variations in these mineral constituents might be accompanied by corresponding variations in the electrical conductivity of water extracts of the flours. This could be postulated, if Swanson's (1912) suggestion that the principal inorganic elements of the ash, potassium and phosphorus are present in part in the water extract as potassium phosphate is correct. To ascertain whether or not such a relation existed, a series of flour samples was collected from a Minnesota flour mill, representing each of the flour streams. There were five break flours, three sizings flours, seven middlings flours, two tailings and three low-grade flours, in addition to the patent, clear, and red-dog flours marketed by the mill. These were employed in the conductivity studies because they afforded a progressive series from the standpoint of grade, with wide differences between the extremes. With the assistance of Miss Anna Peterson the effect of several variations in the technique and method were studied, and it was observed that the temperature at which the extraction was conducted was of greatest significance. The conductivity of extracts maintained at 40° during the period of extraction was materially greater than that extracted at 0°. With a certain flour these values for $\kappa \times 10^{-4}$ at 30° for extracts prepared at 40° and 0° were 4.82 and 6.00 respectively. The proportion of flour to water in the mixture during extraction was of importance, and a ratio of 1 part of flour to 10 parts of water was deemed most satisfactory. The treatment of the extract must be uniform and there was the least variation in the results when the extract was clarified by centrifuging for a few minutes, followed by filtration of the supernatant liquid. If the suspension of flour in water was placed in the conductivity cell without first filtering it, the conductivity gradually increased as the flour particles settled out of suspension. The period of extraction was of less significance than was anticipated, and 15 minutes' continuous shaking in a thermostat at the desired temperature

yielded an extract of practically the same conductivity as 1 hour's extraction.

The method adopted in testing the series of flour samples was essentially as follows: 10 grams of flour were shaken up with 100 c.c. of carefully prepared conductivity water in a Jena glass flask, and the mixture maintained in an ice bath at 0° for one hour. During this time the flask was shaken vigorously every 10 minutes. The contents of the flask were then placed in a tube and whirled for about 5 minutes in a large centrifuge. The supernatant liquid was filtered, returning through the filter until clear, and clear filtrate placed at once in the conductivity cell. The latter was immersed in a water thermostat at 30° and brought to temperature. The conductivity was determined in the conventional manner, the usual and necessary precautions being taken to insure accurate results.

The series of conductivity measurements shown in the following table were made with the collaboration of Mr. E. H. Doherty. The samples have been classified by groups as they are known to the miller. If these are rearranged in order of their ash content it will be found that with the exception of one group, and a single member of another group, the conductivity parallels the ash content. The group which presents the exception is the break flours, four of the five having lower conductivity values than would be expected from their ash content. The large proportion of variation in this group of flours suggests the operation of some factor in the break flours which does not appear in the other flours. The only other variation from the otherwise uniform parallelism between the percentage of ash and conductivity of the water extract is found in the third low grade. The conductivity here is lower than would be computed from the ash content. The data at hand do not indicate the exact reason for these relatively small deviations.

The possible value of this test of flour grade is indicated by this preliminary investigation. The determinations can be made with ease and speed when the equipment is assembled, and the technique acquired. We propose to carry the study farther when research of this character can properly be resumed. In the interval it seemed advisable to present these findings, that they may be applied if the method proves to be as well suited to this purpose as appears at this time.

Specific Conductivity of Flour Extracts at 30°

Grade of Flour	Conductivity $\kappa \times 10^{-4}$	Ash, Per Cent.
First break	6.15	0.41
Second break	5.69	.34
Third break	5.78	.34
Fourth break	6.83	.91
Fifth break	9.56	1.50
First sizings	5.25	.53
Second sizings	5.49	.58
Third sizings	6.91	.71
First middlings	4.61	.38
Second middlings	4.51	.39
Third middlings	4.50	.38
Fourth middlings	5.18	.44
Fifth middlings	5.25	.53
Sixth middlings	6.11	.62
Seventh middlings	6.30	.67
First tailings	7.71	.97
Second tailings	9.18	1.27
First low grade	6.72	.79
Second low grade	7.59	.93
Third low grade	7.53	1.07
Patent	5.21	.45
Clear	7.71	.90
Red dog	14.98	2.53

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THE NORTH CAROLINA ACADEMY OF SCIENCE

The North Carolina Academy of Science held its seventeenth annual meeting at the State Normal College, Greensboro, on Friday and Saturday, April 26 and 27, 1918. The executive committee met at 2:10 P.M. on Friday and passed on the business matters of the academy. The reading of papers was begun at 2:45 P.M. and continued until 5 P.M., when adjournment was had. At night owing to the absence of President W. A. Withers, due to serious illness in his family, the presidential address on "Gossypol" had to be omitted. How-

ever, Professor W. C. Coker gave two papers with lantern-slide illustrations—"*Azalea atlantica* and variety" and "A visit to Smith's Island." Smith's Island is situated at the mouth of the Cape Fear River and is of especial interest because it is the northern limit of various subtropical forms, particularly the palmetto palm. The academy then adjourned to another building where it was tendered a reception by the faculty of the college and the senior students in the science and home economics courses.

The academy met in annual business session at 9:10 Saturday morning. The minutes of the last meeting were read and approved, as were the reports of the Secretary-Treasurer and various committees, especially that on the teaching of science committee was continued for another year. The membership on January 1, 1917, was reported to be in the high schools of North Carolina. This latter 88. During the year 13 members were lost by resignation, removal from the state, and other causes, and ten new members were elected, bringing the membership on January 1, 1918, to 85. Five new members were elected at this meeting. An invitation from Trinity College, Durham, for the academy to be its guest at the next annual meeting was accepted.

The following officers were elected for 1918–19:

President—E. W. Gudger, State Normal College, Greensboro.

Vice-president—H. B. Arbuckle, Davidson College, Davidson.

Secretary-Treasurer—Bert Cunningham, Trinity College, Durham.

Additional Members Executive Committee—George W. Lay, St. Mary's School, Raleigh; Miss Gertrude Mendenhall, State Normal College, Greensboro; J. J. Wolfe, Trinity College, Durham.

At the close of the business meeting, President Foust, of the college, informally welcomed the academy to the college. Next a joint meeting was had of the academy and the North Carolina Section of the American Chemical Society, at which the chemical papers of general interest from the chemists' program were read. Following this, each organization went into separate session. Adjournment was had at 12:50 and the visiting scientists as guests of the college were entertained at luncheon in the college dining hall. Reconvening at 2 P.M., the reading of papers was concluded at 2:30, at which time the academy adjourned.

The effects of the war on the academy were plainly to be seen in the smaller number of papers than usual presented, and in the large number of resignations sent in or pending due to service in the army. Out of a total enrollment on January 1, 1918, of 85, 9, or over 10 per cent., are in the war, and others, particularly chemists, have gone north to engage in war work. However, there was an attendance of 24, and the meeting was a very enthusiastic and thoroughly enjoyable one. The smaller number of papers made possible the considerable discussion which followed the reading of nearly every one. The following papers were presented, numbers 4, 7, 12 and 20 of which will appear in the current issue of the *Journal* of the Elisha Mitchell Scientific Society.

The war work of American physicists: C. W. Edwards. (Read by the Secretary.)

Some important but largely neglected scientific facts: George W. Lay.

Symptoms of disease in plants: F. A. Wolfe.

The sun's eclipse, June 8, 1918: question: John F. Lanneau.

Entrance requirements in science at the State Normal College: E. W. Gudger.

Extension of the range of Prunus umbellata into North Carolina: J. S. Holmes.

Eliminations from and additions to the list of North Carolina reptiles and amphibians: C. S. Brimley. (Read by the Secretary.)

Azalea atlantica and variety: W. C. Coker.

Notes on the magnetic compass: T. F. Hickerson.

Variation within the individual sponge towards types of structure characteristic of other species and genera: H. V. Wilson.

New or interesting North Carolina fungi: H. C. Beardslee.

Herpetological fauna of North Carolina compared with that of Virginia: C. S. Brimley. (Read by the Secretary.)

Further occurrence of cross conjugation in Spirogyra: Bert Cunningham. (Lantern.)

A visit to Smith's Island: W. C. Coker. (Lantern.)

Some methods and results of a plankton investigation of Chesapeake Bay: J. J. Wolfe and Bert Cunningham. (Lantern.)

Mineral fertilizers; their mode of occurrence and distribution in North Carolina: Collier Cobb.

Notes on buds: E. W. Gudger.

Recent changes in Currituck Sound: Collier Cobb.

The return shock due to lightning: Andrew H. Patterson.

Report of investigations on the cause of death of matured chicks in shell in artificial incubation: H. B. Arbuckle.

E. W. Gudger,
Secretary